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Let researchers try new paths

Demand for steady outputs stymies science. If scientists are to pursue the most important research, the ability to pivot is essential, say Tolu Oni, Fabio Sciarrino, Gerardo Adesso, and Rob Knight

The scientific enterprise is staggering under a Catch-22 situation. Scientists are charged with advancing promising new questions, but receive support and credit only for revisiting their past work. While studying the epidemiology of HIV and tuberculosis, one of us (T.O.) realized that many of her urban patients also suffered from non-infectious diseases like hypertension and obesity, but that hardly anyone was investigating the patterns and causes of co-existence of these diseases or strategies for prevention and management. When she proposed shifting her research to this topic, peer reviewers discredited her proposals because she had not asked such questions before.

We, the authors of this Comment, met earlier this year after being selected by the World Economic Forum as part of a group of young scientists who are expanding the frontiers of science. This article is a result of many hours of discussion during and after the meeting, when we found that despite the recognition we have achieved and the diverse disciplines and geographic regions we represent, we share many challenges.

Most striking are the barriers we face to achieving impact. Our research often led us to questions with a greater potential impact than our original focus, typically because these questions encompassed complexities faced by humanity and society. We realized that shifting topics would lead to more important work, but policies of research funders and institutions consistently discourage such pivots.

Shackled to the past

When reviewers assess grants or academic performance, they focus largely on our track records in a particular field. Scientists, who must focus on developing their own careers, are thus discouraged from exploration. Our own experiences provide a brief glimpse into the well-intentioned forces that can keep researchers from trying new paths. (See ‘Turning power’)

This challenge is not new. Venkatraman Ramakrishnan, President of the Royal Society, worked for several years in a job with funding that was contingent on a steady stream of publications, which forced him to ask “safe” but incremental questions. To pursue what became his Nobel-prize-winning research, he moved to another institution where he could ask the questions that interested him, irrespective of the chances for publication. The decision required an international move and a large pay cut.

For every story like this, there are too many where investigators have made a rational decision not to explore areas outside their core expertise. We end up spending so much effort

trying to find our way that we risk losing our drive to apply our skills to the broader world and stick instead to the safe path of "productivity".

This problem is more acute than ever. Earlier this year, Eva Alisic, a psychologist and senior research fellow at Monash University Accident Research Centre, began studying how refugee children from places like Syria cope with trauma. Though her research institute has supported her thus far, should this research focus put her career trajectory at risk, she has determined that she would rather give up a traditional academic career than this line of research. The academic endeavour is compromised if we feel that we must leave academia to better contribute to society.

Gaining freedom

Enabling early-career researchers to change trajectories is necessary to encourage the highest impact research. Theories of brain plasticity and team productivity support this. Diverse experiences, alongside specialization, foster new discoveries and foster decision-making skills needed to lead research. In today's complex world, the need for innovative science is greater than ever.

Grant programmes do exist in some parts of the world to promote high-risk projects for high-profile early- and mid-career researchers. Examples include the European Research Council's Starting and Consolidator Grants and the International Early Career Scientist Program jointly funded by the Howard Hughes Medical Institute, Gates Foundation, Wellcome Trust and Calouste Gulbenkian Foundation.

But these small pockets of funding are not enough. Although it is logical to assess a researcher's body of work over time, universities, local research councils and other funding bodies should create a formal mechanism that explicitly supports pivots. If candidates can provide a convincing case for new questions and their own credibility, they should be able to get support.

Make space for a 'pivot narrative': Applications should give researchers in the midst of a shift an opportunity to describe their rationale. In such cases, the significance and potential of the proposed work should be assessed alongside the researcher's proven abilities for research in other fields. Alisic, for example, could explain how her work with youth sensitized her to a growing need for evidence-based interventions to better treat trauma in children fleeing conflict. A 'pivot narrative' would also explain dry spells and the lack of a track record in the proposed area, and so reduce their career toll. Just the simple step of adding a text box to an application form could expand scientists' willingness to explore and assessors' ability to support such exploration.

Revise peer review: There is little to no emphasis on providing peer review training. Equipping scientists with more skills for more nuanced peer review will help them to consider varied

attributes, particularly how to address complex societal challenges and to evaluate broader interdisciplinary questions. This could eventually change institutional cultures.

Although research programmes that arise from pivots might be considered high-risk, we believe that the greatest risk is to stifle innovation by failing to invest in high-achieving emerging scientists, who are approaching the peak of their creativity.

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Turning power

Scientists are discouraged from exploration to focus on developing their careers.

I had amassed specialized expertise in quantum information theory since my PhD, but soon found myself more attracted towards broader and more fundamental questions at the border between classical and quantum mechanics. However, these new interests were met with a lukewarm reception in a national funding landscape more biased towards applied research. I got funding only by turning to unconventional organizations, such as the Foundational Questions Institute (fqxi.org). Soon after, I was rewarded with a series of high impact publications and a substantial follow-up grant from the European Research Council, along the very lines for which I had previously struggled for support. –GA

When I was setting up his lab, colleagues advised me to focus on one microbe rather than the ecosystem of microbes in the gut. After all, my previous work on *Salmonella* was highly topical and thought to have excellent potential for federal funding, and therefore an excellent investment of my startup funds. One of my first graduate students, Cathy Lozupone, cemented my decision to pivot against the advice of evaluation committees and senior colleagues. Though we both knew it was a gamble, she opted to work on bioinformatics and phylogeny despite having no training in computer science. Her software, UniFrac, has now been cited over 2000 times, and microbiome research has become one of the hottest areas of biomedical research. –RK

To conduct urban health research, I required time to explore the field and engage with new sectors of academia, society, and policy. I also needed additional training in spatial analytical tools to

better investigate health inequities and their urban determinants. At the same time, my lack of publications in the field made me less competitive for grants. I continued publishing on my earlier infectious diseases work but has faced concerns that my new focus is ‘diluting’ my research record. A faculty position in a department has offered support and flexibility to pursue this chosen focus, but work is slow. –TO

Since my PhD, I have been working on the foundations of quantum mechanics and experimental quantum optics. With the quest for a long-term vision, my interests moved to merging my previous research with a more technological approach, namely innovative integrated photonics. This transition was hard to achieve: several grant applications on this idea were not funded Nevertheless, a new PhD student, supported by a University fellowship, believed in that project: Having this student enabled me to carry out a simple, but conceptually relevant, proof-of-principle demonstration of a quantum chip. This discovery was a key ingredient for the award of a grant from the European Research Council (www.3dquest.eu) that allowed me to achieve breakthroughs in this area, now a hot topic within the second quantum revolution. – FS